further observation, and will at last enable us to arrive at the antiquity of man within certain probable limits of error. Without laying stress on any portion of the above very rude estimate, it may, I think, be averred that it is not palpably too high, but is just as likely to be too low; and this last supposition will be rendered more probable when we consider the vast lapse of time implied by the position of some of the recently discoverd palæolithic weapons.

The flint tools found in the gravel at Bournemouth, in the Isle of Wight, and near Salisbury, at elevations of from 80 to 100 feet above the present valleys, imply, according to the best observers, that the whole series of surrounding river valleys have been excavated since they were deposited, and that the system of drainage and position of the coast-line have been very greatly altered. The hippopotamus of the Gower Caves implies changes equally great, since the peninsula of Gower now contains only small streams, and could not possibly have had a large river without very important changes in its relations to the adjacent country. The position of the flint weapons in the valley of the Somme, at Hoxne in Suffolk, and in many other places, all combine in indicating that very important changes in physical geography have taken place since they were deposited. We can hardly suppose that in all these different localities the changes were abnormally rapid, especially as in no case do records of the historic period indicate that any remnant of the process was then going on; and from what we do know of the rate of such changes, and their intermittent nature, we are entitled to affirm that the most extreme estimates yet made of the antiquity of the men who fashioned and used the palæolithic implements is quite as likely to be under as over the truth.

There is as yet no clear evidence that man lived in Northern Europe before the glacial epoch, and even if he did so the action of the ice sheet would probably have obliterated all records of his existence. Every evolutionist, however, now believes that he must have existed far back in the tertiary period, and that the proof of it will be found, if at all, in some of the warmer regions of the old world. Here is surely a problem of grand and absorbing interest awaiting solution at our hands. Geologists are not usually wanting in energy or enterprise, and they number in their ranks many wealthy men. It is to be hoped that they will soon energetically attack the problem; and no more promising field of research offers itself than the limestone caves of Borneo, which can be explored with perfect safety, and at a moderate expense. We can hardly now expect any great additions to our knowledge respecting the antiquity of man in Northern and Central Europe, and must go to warmer regions if we wish for new discoveries and startling revelations.

A. R. WALLACE

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Fellowship at Magdalen College

I THINK the notice in NATURE of Sept. 25 respecting the election about to take place to a Natural Science Fellowship at Magdalen College requires some comment. The amount of academic preferment which falls to the share of science in Oxford is so small, that it might reasonably be demanded that what there is should be thrown open to as many candidates as possible. When, therefore, it was announced that the Fellowship would be given for proficiency in Biology, it might have been inferred that the electors had this object in view. Biology is held, elsewhere than in Oxford, to be the science which treats of the laws governing organization and vital activity; in other words, structure and function in *all* forms of life, whether vegetable or animal. It was not, perhaps, an unreasonable inference, therefore, to draw from the terms of the notice, that it was the intention of the College to make Biology in its widest sense the foundation of the examination, and to allow individual candidates to exhibit, in addition, such detailed knowledge as they might possess of Zoology, Botany, or even Palæontology. This would not have attributed to Biology a wider meaning than, for example, Mr. Herbert Spencer or the Science and Art Department attach to it. Thinking it desirable, however, to get some official information upon the subject, I wrote to the President, who, after some delay, replied that, in his opinion, as preference would be given to Biology, it would be useless to offer Botany as a special subject. This is not more reasonable than it would be to say, that because Physics was to be the subject of an examination it would be useless to offer Electricity or Heat as a special subject. But the terms of the President's reply were rather ambiguous, and I therefore made some further inquiries. I learnt, as the result, that the College considered it impossible to compare the merits of a candidate who stood on the Zoological, with one who stood on the Botanical, side of the general subject.

I think myself the difficulty is not one which should have been found insuperable; but, assuming that the College had sufficient grounds for a different opinion, then I think the electors should not have offered their Fellowship for Biology, when what they really had in view appears to be a detailed knowledge of the Zoological preparations in the University Museum.

W. T. THISELTON DYER

The Sphygmograph

THERE appears in NATURE, vol. viii. p. 330, a notice of a thesis for the M.D. Cantab. on the subject of Bright's disease, in which reference is especially made to some sphygmographic observations therein contained. It is apparently from the pen of Mr. Garrod, who is himself the author of interesting and important researches with the sphygmograph and cardiograph. While agreeing with a part of my explanation of the normal pulse tracing, as regards the points in which it differs from the view commonly received, he takes exception to the account which I have given of the tidal or first secondary wave. It may be well to say in reply a few words upon the point at issue, since the reference to it in the thesis was very brief and incidental, and I should not wish it to be taken as a full account of my views as to the mechanism of the pulse.

The explanation of Mr. Garrod himself is that the tidal wave is an instantaneous wave due to the closure of the aortic valves. This theory was first proposed by M. Marey to account for the tidal wave in many of its forms; but, so far as I know, it has not been adopted by any writer on the subject in England with the exception of Mr. Garrod. There is this difference, however, between them, that while M. Marey holds that the dicrotic wave has nothing to do with the aortic valves, but is a reflection from the periphery, Mr. Garrod considers that it is the wave of expansion from the closure of the aortic valves, which becomes separated from the instantaneous wave as it recedes from the heart. Thus the faculty of originating two waves of different velocity, which by most writers is attributed to the first impulse of the heart, combined with the closure of the mitral valve, is by Mr. Garrod denied to that event, but ascribed to the closure of the aortic valves. Now I believe it to be mechanically impossible for any wave to be propagated with a velocity different from that of the wave of expansion, except the purely vibratory wave of sound, and Mr. Garrod appears himself to hold that a mere vibration produces no elevation in the tracing. The question, however, may easily be determined experimentally. If there appear in the tracing two waves which are travelling with different velocities, their relative position will vary at different distances from the heart. Let, therefore, anyone who wishes to settle the question for himself take tracings of a good many

© 1873 Nature Publishing Group

markedly tricrotic pulses, say from the femoral and also from the dorsalis pedis arteries. According to the view of Dr. Burdon Sanderson and most other writers, the interval between the primary and tidal waves ought to be more than doubled in the dorsalis pedis; according to the view of Mr. Garrod, on the contrary, that between the tidal and dicrotic waves. It will be found that there is no such considerable and constant variation as would be required by either theory, although the tidal wave does not maintain its relative position so closely as does the dicrotic wave. The kind of pulse best of all suited for this experiment is fortunately * rather scarce; it is that of a young person who has a granular kidney, but is free from dropsy.

The theory of Mr. Garrod may appear at first sight suitable to one of the forms of healthy pulse, in which the tidal wave appears as a slight elevation preceding the dicrotic wave; but I do not think that it will be accepted by anyone who has watched its variation in a large number of diseased pulses, and has seen it pass through every gradation, from a separate and distinct wave to a mere convexity in the descending curve, which may commence immediately from the top of the primary upstroke. In the pulse of rigid arteries this latter form is often taken when the heart is quiet, but when it acts more vigorously the tidal wave becomes separated, owing to the development of the so-called "percussion element," which is really the effect of acquired velocity in the sphygmograph. The case which should afford the most crucial test is perhaps that very rare one in which the aortic orifice is closely obstructed, and scarcely any valves remain to produce a wave by their closure. The tidal wave should then, according to Mr. Garrod's theory, be at least greatly diminished, but, in point of fact, it is then more greatly developed than under any other circumstances whatever. Evidence to the same effect may be derived from the use of an artificial heart with experimental elastic tubes, for it is found that, under suitable conditions, the tidal wave may be greatly prolonged by a protracted contraction of the heart. This was first shown by Mr. Mahomed in the *Medical Times*, and although I believe his theory to be erroneous as to the relation between the primary and tidal waves, yet, with regard to the practical associations of the tidal wave, my experiments have led me to conclusions which are quite in agreement with his, namely, that three things contribute to the development of the tidal wave—increase of pressure, diminution of elasticity, and prolongation of the heart's contraction.

Mr. Garrod argues that the tidal wave cannot have anything to do with the inertia of the long lever, because it is shown in the reflecting sphygmoscope, in which that is absent. I do not, however, consider that the result is due solely, and possibly not even chiefly, to the inertia of the lever, but to that of the instrument altogether, and inertia is possessed likewise by the sphygmoscope. Moreover, since the latter does not record its indications, it would be difficult to ascertain whether the tidal wave shown by it corresponds precisely to that of the sphygmographic tracing. Another instrument has also been called a sphygmoscope, in which the motion of the pulse is shown by the variation of a gas flame. In this there appears indeed the counterpart of the tidal wave, but not in the form of a single wave; instead of this a series of small waves is shown. These may appear only as a slight quivering motion, and are evidently due to the oscillation of the elastic diaphragm upon which the pressure of the pulse is received.

Mr. Garrod maintains his own theory especially on the ground of observations with his cardio-sphygmograph, showing the commencement of the tidal wave in the radial pulse to be synchronous with the closure of the aortic valves. But the determination of the moment of that closure depends on the correctness of his interpretation of the minor elevations in the cardiographic tracing. These are numerous, and his interpretation of them all is most ingenious, but to accept it requires an implicit faith that the instrument itself has no part in producing any of the minor features of the curve. Now, that curve was drawn by a lever, moving on a pivot, and balanced between two springs, which would seem a contrivance peculiarly liable to oscillate. When therefore it is further found that in cardiac tracings published by other observers, or those obtained by applying the sphygmograph directly to the heart, there is no close correspondence either in the number or the position of the elevations, the conclusion can hardly be resisted that some of them are due to such oscillation. My own opinion is that neither in the cardiographic nor in the radial pulse tracing can the point corresponding to the end of systole be precisely determined.

The whole subject is one which it is difficult even to state intelligibly without a constant reference to diagrams of tracings, and therefore, for a fuller account of my views as to the theory of the pulse, particularly in reference to the complete explanation of the dicrotic wave, I must refer to a paper to be published in the next number of the *Journal of Anatomy and Physiology.*

While I consider that the construction of the sphygmograph has some influence on the tracing produced, yet I believe that, by a fortunate chance, the result is more practically useful than if the pulse-wave were recorded with perfect accuracy, for I think that slight differences in it, which would then perhaps escape notice, are, as it were, magnified and made manifest to the eye.

I may say in conclusion that I do not quite agree in the view that we must wait for the practical application of the sphygmograph until physiologists are agreed about the theory of the pulse, for, according to present appearances, that consummation is distant indeed. There is, however, among sphygmographers an agreement about practical inferences which is almost as notable as the confusion which prevails as to mechanical causes. It is possible therefore for a person to use the sphygmograph for diagnosis and prognosis, who does not even attempt to understand the cause of the waves seen in its tracings. But it must be allowed that the settling of the mechanical question is much to be desired, and that, without it, the sphygmograph cannot afford that service, which otherwise it would be capable of doing, to the solving of all general physiological problems relating to the vascular system. And, from a practical point of view, these may perhaps be regarded as among the most important in physiology, for it is probably through the agency of the vascular system that many of the greatest effects of remedies are produced.

A. L. GALABIN

On the Origin of Nerve-Force

IN a paper on this subject, by Mr. A. H. Garrod, in NATURE, vol. viii. p. 265, the author states that in cold-blooded animals, nerve-force must be generated by the difference between their own temperatures and that of the medium by which they are surrounded. Now, to take the case of a frog as a common example of a "so-called" cold-blooded animal: A few days ago, when the thermometer was standing at 71°, I took the temperature of two frogs, one was 69°, and the other 67°; the difference between their temperature and that of the surrounding air was practically *nil*. Now, on a day of this sort of temperature, it would seem that the pervious integument of the frog is continually exhaling moisture, and that in consequence the temperature falls, and would continue to fall below that of the surrounding air, were it not that it was raised by the heat generated "by the destruction of tissue that is continually going on within the body of the animal;" so between these two contending forces a state of equilibrium results, and the temperature of the animal and the surrounding air are the same. But, if this be true, it follows that the whole of the heat from the animal is used up in keeping up its temperature, and therefore none can be spared for conversion into nerve-force. Therefore, a frog at rest on a summer's day ought to have no nervous energy. Now, suppose our frog takes to leaping vigorously, he will develop a certain amount of heat, and then he ought to have a great deal of nerve-force; but it is not found that an active frog is more "nervous" than a quiescent one.

Again, the nervous irritability of a frog, though perhaps not acting with the instantaneous energy with which it acts in a mammal, still persists far longer than in other vertebrates, and will continue much longer after the somatic death of the animal, when it is quite clear that the temperature of the body and the surrounding medium will be the same. Now in this case the nerves may be so irritated as to lose all irritability, and yet, after a period of rest, this irritability will be regained, clearly, to my mind, showing that nervous energy must be generated after the death of the animal, when all differences of temperature have ceased.

Finally, it must be admitted, without the aid of any hypothesis, that the difference between the temperature of a frog and the surrounding air is, at any time, very slight; and yet this animal possesses what we call an extremely "persistent" form of nerve-force.

R. LYDEKKER

---

* [We have omitted the prefix *un-* from this word; we hope Mr. Galabin will forgive us.—ED.]

© 1873 Nature Publishing Group